BEFORE THE STATE OF WASHINGTON
ENERGY FACILITY SITE EVALUATION COUNCIL

In the Matter of Application No. 99-1:

SUMAS ENERGY 2 GENERATION FACILITY

EXHIBIT ___ (MM-RT)

APPLICANT'S PRE-FILED REBUTTAL TESTIMONY

WITNESS: MARK MOLINARI

Q. Would you please reintroduce yourself to the Council.

A. My name is Mark Molinari. I am an Associate Engineering Geologist with URS Corporation and a professional geologic consultant.

Q. What subjects do you intend to address in your testimony?

A. I will be responding to the written testimony of Professors Don Easterbrook ("Easterbrook PFT") and David Engebretson filed by Whatcom County on October 1, 2001 ("Engebretson PFT").

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Q. What was your general impression of Professor Easterbrook's testimony?

A. Well, although I expected him to disagree with some of my opinions, I was surprised by his tone. On nearly every page of his response to my testimony, he seems to suggest that I am incompetent, irrational or lack knowledge of geologic principles. *See, e.g.*, Easterbrook PFT, pp. 7:12, 8:4-5, 9:2, 10:16, 10:23, 11:18, and 13:21. I am accustomed to and comfortable with debating the geological data and evidence. However, this is the first time I have felt personally attacked in this way.

Q. Would you like to respond to Professor Easterbrook's comments in this regard?

A. Yes. As to my competence generally, I feel that my resume and the quality of my work over the past 20 years speak for themselves. *See* Exhibit MM-1 to Applicant's Prefiled Testimony: Mark Molinari ("Molinari PFT"). For example, my Masters thesis and associated research was on active faulting. During my professional career I have performed numerous investigations of active faults and evaluated other geologic and seismic hazards for various large industrial facilities such as SE2, as well as more sensitive facilities such as dams, hospitals, schools, a nuclear power plant, etc. These projects have been located throughout the western U.S. as well as internationally.

With respect to this project in particular, I would simply add that I am not the only one to have come to conclusions contrary to those of Professor Easterbrook. First, in evaluating Professor Easterbrook's opinions and assessing the available evidence, I have spoken with other geologists who have done considerable geologic research in the region (*i.e.*, Dr. John Clague at Simon Fraser University, Joe Dragovich at

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Washington Department of Natural Resources, and Dr. Bernard Hallett at the University of Washington, two of whom, incidentally, possess academic creditials equivalent to Dr. Easterbrook) and obtained second opinions regarding my conclusions from co-workers (*e.g.*, Dr. Bob Burk) as well as knowledgeable professionals in other relevant disciplines such as geotechnical and structural engineering. They have uniformly provided information that supported my interpretations and/or agreed with my conclusions. Those that actually reviewed Professor Easterbrook's opinions as to the degree of potential risk from seismic hazards at the SE2 site thought them to be either unsupported by available evidence or a significant exaggeration of the potential risk indicated by the available evidence.

Second, the independent consultants hired by the Council, Jones & Stokes, have reviewed the relevant literature and considered both my opinions and those of Professor Easterbrook and his colleagues. Generally speaking, they have come to the same conclusions that I have. For example, with respect to the potential for fault rupture at the site, Jones & Stokes conclude that "[t]he potential for damage to the plant site or pipeline by fault rupture is considered ***highly*** unlikely because of the lack of any evidence of geologically recent surface faulting in the project vicinity." *Id*., p. 3.7-8 (emphasis add). They further conclude that even "if surface rupture were to occur[,] it would not directly affect the S2GF facilities because they do not overlie the trace of the fault as inferred by Easterbrook et al." *Id*. p. 3.7-9. Likewise, with regard to Professor Easterbrook's concerns about the potential for landslides, Jones & Stokes conclude that since "[t]he project site is situated in a broad flat-lying valley" and "[t]he topography on and near the site consists of stable natural slopes with less than 5 percent grade . . .

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seismically induced slope failures are not a consideration at the site or along the pipeline corridor." *Id.*

That said, I do not mean to suggest that the Council should attempt to resolve issues regarding possible seismic risks at the SE2 site or any other site by simply counting the number of opinions on one side or the other of the debate, or by simply adding up the number of publications or years of experience of one expert as opposed to another. In geology, as in other scientific and professional disciplines, there is commonly more than one interpretation of the available data, the basis of which should be clearly stated and evaluated on a rational basis. I therefore trust that in evaluating opinions on seismic hazards, Professor Easterbrook's as well as my own, the Council will ignore any rhetoric and carefully consider the weight of the evidence and the logic of the reasoning. I am confident that once it has done so, the Council will conclude, as I have, that the potential seismic risks at the SE2 site are not sufficient to make it unsuitable for an energy generation facility such as that proposed by SE2, provided that the appropriate design studies and mitigation measures are implemented in accordance with current standards of professional practice.

Q. At the beginning of his testimony, Professor Easterbrook reiterates the conclusions from the "preliminary" paper he authored with Professor Engebretson and Dori Kovanen. Do you have any further comments to make regarding these conclusions?

A. I have already addressed this report and its conclusions in considerable detail in my earlier prefiled testimony and the exhibits attached thereto. In my prefiled testimony

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as well as within this rebuttal testimony and the associated exhibits, I have tried to present the pertinent, readily available geologic data and information and demonstrate how it is inconsistent with Drs. Easterbrook and Engebretson's conclusions. I would like to note, however, that for the most part, they have not specifically addressed the evidence or the reasoning underlying my opinions. Instead, he has mostly expressed outrage at my opinions and just re-asserted his conclusions.

For example, Dr. Easterbrook again states that the Vedder Mountain fault and the Sumas Fault are seismically active. *See, e.g.*, Easterbrook PFT, pp. 2:23-3:1. As I pointed out in my earlier testimony, however, Professor Easterbrook does not indicate what criteria he uses to determine whether a fault is active or whether the available data for the Vedder Mountain and Sumas faults meet his criteria. Molinari PFT, pp. 10:1-5. Moreover, I also earlier pointed out that according to the definition used in California, these faults would *not* be considered active. *Id.*, p. 11:9-37. There are hundreds of faults and widespread seismcity in the Puget Sound area, however only a few faults have been shown to be active in the recent geologic past. In assessing activity, data on historical and pre-historical (paleo) seismicity as well as surficial geology are used. Nevertheless, nowhere in his testimony has Professor Easterbrook explained the criteria he is using or showed how the data meets that criteria. Instead, he again merely asserts that the faults are active.

Furthermore, it is important for the Council to understand that there has been considerable research and advancement in the science and engineering of geologic and seismic hazards in the last 30-35 years. This work has been performed by

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academics, state geological agencies, the U.S. Geological Survey, and private consulting firms. With each large earthquake we learn new information and refine our methods and interpretations. This has resulted in modifications of building codes, new construction methods, and an evolution to the current standard of practice where the hazards can be better identified, investigated, and quantified to assess the risk ***probabilistically*** and incorporate the necessary factors into the siting and design of engineered structures of all types and sizes. Dr. Easterbrook appears to be either unfamiliar with the current standard of practice or chooses to ignore it. His conclusions regarding the seismic hazards and relative risk at the SE2 site are based on extreme scenarios with a very low potential to occur during the proposed project life span, and he rejects any notion that engineering and construction methods can mitigate potential hazards at the site.

Q. Regarding the basis for the conclusions in Professors Easterbrook and Engebretson's preliminary report, Professor Engebretson indicates that a University of Washington Geophysics Department web site provides "insights into the seismic characterization of the proposed SE2 site." Engbretson PFT, p. 2:22-23. Do you agree?

A. No. I reviewed the UW web site prior to preparation of the revised application. It is a good source of general information. It provides an overview of the seismic hazards of the Puget Sound region, provides specific information on notable earthquakes, and allows the visitor to search for historical earthquakes by time and area. However, it does not provide any data specific to the Sumas Valley or surrounding area beyond that presented by Professors Easterbrook and Engebretson in their preliminary report.

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Q. Professor Easterbrook reiterates what he considers are the "[f]our principal seismic hazards [that] have been identified" with respect to the SE2 site. Do have any further comments regarding these hazards?

A. I also addressed these issues in detail in my earlier testimony. However, once again, Professor Easterbrook does not address the specific evidence or reasoning that I have cited regarding these issues, and except for a solitary undated map, he does not offer any new evidence or data to contradict the evidence I provide or to further support his conclusions. In addition, his characterization of the potential for these hazards to occur at the site as well as the degree of the hazard (e.g amount of surface displacement) are based on the mere ***possibility*** of occurrence (no matter how remote) and a worst case scenario that is not supported by the site and area specific data, rather than the ***probability*** of occurrence and estimates based on standard engineering and scientific methods used for seismic hazard analyses. Following are several examples.

<u>Ground Shaking</u>. It is true that if there were a moderate to large earthquake on either the Sumas or Vedder Mountain fault it would generate significant strong ground motion at the site. However in assessing the level of the hazard, the probability of this occurring should also be considered. As previously indicated in my prefiled direct testimony and reiterated here, the currently available data indicate there is a relatively low likelihood that these two faults have generated large earthquakes in the recent geologic past. Furthermore there are numerous properly designed and constructed, large structures in similar proximity to active faults in California, as well

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as other seismically active areas of the world, that have experienced strong ground motions from moderate to large historical earthquakes and survived. In addition, downtown Seattle, Bellevue, Bremerton are all located within 2 miles of the active Seattle fault, and large and sensitive structures are located and continue to be built within these areas. Therefore strong ground motion needs to be considered in the design of the proposed SE2 facility but should not preclude siting a facility of this type in Sumas. As discussed in the testimony of my colleague Allan Porush, the expected ground motions at the SE2 site can be readily designed for using standard structural engineering methods.

Liquefaction. With respect to liquefaction, I mentioned that like Professor Easterbrook, I am not a geotechnical engineer and am therefore not an expert in this area. Nevertheless, I work with geotechnical engineers on a regular basis, I have reviewed numerous publications on liquefaction occurrence and susceptibility, and I have conducted preliminary liquefaction susceptibility assessments for numerous other large industrial facilites in seismically active areas. Also, in preparing my previous testimony, I re-reviewed and cited several major peer-reviewed papers on liquefaction, and consulted with geotechnical engineers to confirm that my understanding of the basic principles of liquefaction were correct. One of those basic principles, for which I cited several authoritative references, is that “[c]lay and silt rich soils . . . are typically significantly less susceptible to liquefaction.” Molinari PFT, p. 19:27-31. Without citing any peer-reviewed papers or other research to the contrary, Professor Easterbrook asserts just the opposite, namely, that liquefaction occurs principally in “soils such as silt or clay.” Easterbrook PFT, p. 3:16.

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Similarly, I explained that another basic principle regarding liquefaction specified in the references cited is that even if the soils are the appropriate grain size, liquefaction is "typically limited to saturated soils at depths of 30 to 40 feet or less." Molinari PFT, p. 19:31-33. The reason for this is simply that the weight of the earth that lies on top of soils below these depths tends to compress and densify the soils, thus preventing them from liquefying even if they would otherwise fit the profile for liquefiable soils. *Id.*, p. 19:33-37. In response to Allan Porush's testimony, however, Professor Easterbrook states (again without any supporting references) that commonly used engineering solutions to deal with such situations — *e.g.*, extending piles through the upper layers of soil to the denser, more compressed layers below — are "useless" because "[t]he thicknesses are far in excess of this." Easterbrook PFT, p. 16:23-25.

What Professor Easterbrook means by this is not entirely clear, but he seems to be suggesting that liquefaction could occur hundreds of feet below the ground surface. As mentioned, this is contrary to one of the most basic principles of liquefaction. *See, e.g.*, Tinsley *et al.*, 1985; National Academy Press, 1985; Obermeier and Pond, 1999. Moreover, it is very likely that sediments in the lower portion of the basin include older glacial drift or other deposits that were too dense to liquefy even before burial by the looser, surficial post-Sumas non-glacial deposits. I therefore would have expected him, at a minimum, to cite a contradictory reference or provide a liquefaction analysis using appropriate subsurface data to explain how he reached this

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conclusion. Instead, he simply asserts that the mere possibility of liquefiable soils renders any and all design solutions impossible.

Fault rupture. As I explained in my earlier testimony, fault rupture is not generally considered a significant risk unless there is evidence of an offset along the fault within the last 10,000-11,000 years. In addition, there are numerous studies and compilations showing that significant, primary surface fault ruptures associated with large historical earthquakes in the western U.S. (as well as other areas of the world with similar tectonic settings) have all occurred on faults with evidence of prior surface rupture in the Holocene or latest Pleistocene. Studies have been conducted on many of these faults to assess the timing and nature of pre-historical earthquakes (paleoseismology), and these show that recurrence intervals for surface rupturing events on these faults typically range from hundreds to several thousand years. Therefore, lack of evidence for displacement or deformation of Holocene age deposits is a well established basis for assessing potential fault rupture hazard. Indeed, this is the basis for Jones & Stokes's conclusion that "fault rupture . . . is considered highly unlikely" at the SE2 site. D-SEIS, p. 3.7-9.

Again, Professor Easterbrook does not address the criteria he uses for determining the likelihood of fault rupture. Moreover, he admits that "no previous offsets of the land surface have been yet proven." Easterbrook PFT, p. 4:2. Nevertheless, he still insists that both Jones & Stokes's conclusion and mine are "not scientifically defensible" because the Sumas fault might really be like the Seattle fault or the San Andreas fault (Easterbrook PFT, p. 8:2-24). Professor Easterbrook's opinion that there is a

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"considerable" risk of fault rupture at the SE2 site is, apparently, thus based on: (1) the fact that the Seattle fault has clear and definitive evidence of Holocene age surface displacement; and (2) that somehow (unspecified evidence) the nature of surface rupture on a 30-40 mile long normal fault in Washington could be comparable to the San Andreas fault, one of the longest and most active strike-slip faults in the world that is a boundary between two tectonic plates.

The Sumas fault exhibits no evidence of any surface displacement in the recent geologic past, let alone the 15 to 20 foot displacement he implies could occur at the Sumas site. The nature and timing of surface fault ruptures on the Seattle and San Andreas fault zones have absolutely no bearing on the potential for surface fault rupture on the Sumas fault or the nature of a rupture even assuming it were to occur. Neither of these fault zones are connected with or analogous to the Sumas fault: they each have different types of slip; both have significantly greater total amounts and rates of displacement; and both have evidence of repeated surface displacement during the Holocene. It should also be noted that although southern Bainbridge Island has been uplifted 21 feet, the actual surface fault rupture on individual faults within the zone are significantly less than that. I know this first hand because I have participated with the U.S. Geological Survey geologists in the paleoseismic investigations of the two known faults with surface rupture associated with the large earthquake that occurred 1,100 years ago.

Landslides. Professor Easterbrook continues to believe that a seismically induced landslide poses a "possible" hazard at the SE2 site. Similar to his analogy to the San

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Andreas fault, he seeks to substantiate his concern by noting several large pre-historic landslides that he and his colleagues have identified in the Cascade Mountains in the eastern portion of Whatcom County that they are “reasonably sure” were generated by “ancient earthquakes.” Easterbrook PFT, p. 4:7-8. He provides no evidence or information indicating that similar prehistoric landslides of the magnitude necessary to impact the proposed site have occurred in the Sumas Valley or that the appropriate geologic conditions exist for this to occur in the life span of the proposed SE2 plant. To the contrary, later in his testimony he essentially admits that this is not, in fact, a realistic hazard with respect to the SE2 site by stating that “[w]e do not believe that the seismically-induced landslide potential is prohibitively high at Sumas . . . .” *Id*., p. 9:8. Nevertheless, despite the lack of any evidence for prior landslides or geologic conditions favorable for landslides that could affect the site, and his acknowledgement that there is minimal risk, he continues to insist that it poses a “possible hazard” to the SE2 site. Easterbrook PFT, p. 4:4-10. (For another opinion contrary to Dr. Easterbrook’s in this regard, see the D-SEIS, p. 3.7-9, where Jones & Stokes states that “seismically induced slope failures are not a consideration at the site . . . .”)

Q. Professors Easterbrook and Engebretson say that the seismic risks are greater at Sumas than at other places in the Puget Lowland and that in fact, the SE2 site is “unique.” Do you agree?

A. No. As I previously stated, much of Puget Sound has had a level of historical seismicity similar to or greater than the Sumas area. Dr. Engebretson also confirmed this in our interview. There are other documented faults in Puget Sound with

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significant evidence that definitively shows or is strongly suggestive of displacement during the Holocene such as the Seattle fault, the Devils Mountain fault, and several faults on Whidbey Island (*see, e.g*., Bucknam *et al*., 1992; Johnson *et al*., 1994, 1996, 1999, 2000). There is no similar, well documented evidence for either the Vedder Mountain or Sumas faults.

As documented by numerous researchers, most of Puget Sound is underlain by unconsolidated glacial and non-glacial deposits. These are typically several hundred to several thousand feet thick (e.g. the Seattle basin has up to 3,000 feet of Quaternary sediments (Yount et al, 1985; Jones, 1996)) and overlie Miocene age and older rocks. In the upland areas, the near surface unconsolidated deposits were compacted by glacial ice during the Vashon glacial advance and thus do not have a potential for liquefaction. However, near surface sediments in all the major river valleys have a liquefaction potential similar to the Sumas Valley.

As discussed in more detail by Allan Porush, amplification of strong ground motions associated with earthquakes can occur in areas with loose or soft soils or as a result of other subsurface effects. However, it is not uniform phenomena. For example, the U.S. Geological Survey recorded amplified ground motions in various parts of the Seattle metropolitan area during the Kingdome implosion and the Nisqually earthquake, some of which are and some of which are not underlain by loose or soft soils. There are numerous ongoing studies in the Seattle area, as well as in California, by the U.S. Geological Survey and other researchers to try to better estimate under what conditions and how much amplification should be considered for

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seismic design for different subsurface conditions. Consequently, in light of the current state of our knowledge as to this phenomenon, there is no basis for concluding that the Sumas Valley would experience higher amplified ground motions than other areas of Puget Sound underlain by loose, saturated glacial and post-glacial unconsolidated deposits, as implied by Drs. Easterbrook and Engebretson.

Q. On pages 5 through 13 of his testimony, Professor Easterbrook characterizes what he says are the 23 opinions in your earlier prefiled testimony and then sets out his responses to each of those opinions. Would you care to comment on his responses?

A. Yes. I'll take each of his responses in the order he lists them.

<u>1. Requests for additional information</u>.

When Dr. Easterbrook first made his opinions public in November 2000, I was asked to evaluate them by Sumas Energy 2. In my professional opinion, his conclusions regarding the potential seismic hazards at the SE2 site were inconsistent with or unsubstantiated by the published documents I had reviewed in preparation of the revised application, and there was not sufficient information or new data presented by Dr. Easterbrook in his affidavit or his preliminary paper attached to the affidavit to support his conclusions. Based on the nature of his assertions, I expected that he had done sufficient research and analysis to provide the type of detailed information typically presented in a peer reviewed professional journal article or other professional publication to support such conclusions. Their preliminary report (Exhibit DEJ-2) does not meet the standards of such a publication.

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Therefore on my behalf, Sumas Energy requested via the appropriate parties that additional and more specific data be provided. I do not know the specifics of conversations or correspondence between SE2 and Whatcom County, or Dr. Easterbrook and Whatcom County, but it is my understanding that more than one request was made and no new information was provided. It was not until early June 2001, shortly before the revised application was submitted, that I was afforded the opportunity to interview Dr. Easterbrook and Dr. Engebretson personally. Both of them answered my questions and provided additional explanations of the studies performed to date, in particular the historical seismicity analysis by Dr. Engebretson and Lori Roberts, and reiterated their previously stated opinions. Dr. Easterbrook stated that his study to date was preliminary, that he planned more study in the future, and that there were no maps or other more detailed data that could be provided at that time.

2 & 3. Prior mapping and existence of the Sumas fault.

My statement regarding the prior mapping of the Sumas fault was based on the recent U.S. Geological Survey publications referenced in my testimony (Jones, 1996; Cox and Kahle, 1999), as well as a 2000 map published by the Washington Department of Natural Resources (Lapen, 2000) and a 1996 Geological Survey of Canada report on seismic hazards in southwestern British Columbia (Clague, 1996). A report on the subsurface geology in southwestern British Columbia by Gordy (1988) was also reviewed. It is based on oil well and proprietary oil industry geophysical surveys. None of these maps or reports show or discuss the Sumas fault. In addition, as

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mentioned above, when I met with Dr. Easterbrook in June of this year, he specifically informed me that he had no maps apart from those referenced in his preliminary report to substantiate their opinions. My statement was thus based both upon recent publications and Dr. Easterbrook's own assurance that there was no further information available to evidence their claims. It therefore was not without basis, and Dr. Easterbrook's assertion that I am the only geologist who did not acknowledge the fault's existence and that it had been known since he initiated his career 42 years ago is insulting and unfounded.

As pointed out by Dr. Easterbrook, there are other recent (1994 and later) Canadian maps and documents (Mustard and Rouse, 1994; Mustard and others, 1998; Clague, 1998) that I had not reviewed at the time of my direct testimony showing that the Sumas fault is present in the subsurface Eocene age rocks that underlie Sumas Valley. I have reviewed these reports and now concur that the Sumas fault is present in the subsurface rock. However these documents also support my prior interpretation that there is not available evidence that the Sumas fault displaces latest Pleistocene or Holocene age deposits. As stated in Mustard and Rouse (1994), (1) new seismic and gravity (geophysical) data more recent than that reviewed by Gordy (1988) allowed recognition of the Sumas fault, and (2) both the Sumas and Vedder Mountain faults "appear to be confined to the middle Tertiary and older strata (pre-Pliocene?)" and have displacement of the Tertiary rocks of " only a few hundred meters". A geologic cross-section by Mustard et al. (1998) reproduced as Exhibit MM-2 shows that the Sumas fault does not extend above the top of the Tertiary rocks.

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4 & 11. Seismic activity of the Vedder Mountain and Sumas faults.

My point with regard to the prior studies by Dr. Engebretson and Lori Roberts was not to refute the data they present or their interpretations that several small instrumentally recorded earthquakes may be associated with the Vedder Mountain and/or Sumas fault. However, I do feel the pre-instrumental and instrumental data, and the uncertainty of the data, should be put into the proper context with respect to Drs. Easterbrook and Engebretson's conclusion that because of the historical seismicity in the area, the Vedder Mountain and Sumas faults have a significant potential to generate large magnitude earthquakes.

As shown on Figure 3.1-6 (Cross-section B-B') in the Revised Application and on new Exhibit MM-3, shallow low-magnitude seismicity similar to that identified for the Sumas Valley occurs throughout the Puget Sound region. In their analysis of the regional seismicity, Weaver and Shedlock (1996) indicate that "[i]n the Puget Sound basin, crustal earthquakes do not occur along simple, linear fault zones but appear to be distributed throughout the crust." More detailed and/or recent studies and subsequent seismicity indicate three known or probable fault zones associated with local areas with relatively high rates of shallow seismicity: the Darrington seismic zone (Zollweg and Johnson, 1989), the McCauley thrust near Deming in Whatcom County (Dragovich et al., 1997), and the Seattle fault. The seismicity in the Sumas Valley area inferred to be associated with the Vedder Mountain and/or Sumas faults is considerably less than these three zones.

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Therefore, I believe the existing seismicity data for Sumas Valley is suggestive but not conclusive. While it is technically correct that earthquakes are created by fault movement, a few small earthquakes that appear to be associated with a fault do not necessarily indicate that the fault has a significant potential to generate a large earthquake. If this was true, the distributed "background" seismicity recorded throughout the Puget Sound region would suggest that active faults capable of large earthquakes occur throughout the region, and we know that this is not the case.

<u>5, 6 & 10. Surface trace of the Sumas fault and the risk of fault rupture</u>.

I agree that there is an escarpment on the west side of the Sumas Valley, west of the SE2 site. In Canada, much of the escarpment is in rock and glacial deposits whereas in Washington, it is in glacial deposits. However, an escarpment can be caused by faulting and/or erosion and, as previously discussed, there is currently no direct evidence that there has been Quaternary displacement on the Sumas fault along the main topographic escarpment. The two locations that Dr. Easterbrook indicated as being post-glacial escarpments that he suspected are not in line with the mapped, inferred trace of the Sumas fault. Instead, they are more east-west trending and located west of the mapped trace. Consequently characterizing the escarpment as a "fault" scarp, as this term is typically used in tectonic geomorphology, is incorrect.

The potential risk of fault rupture at SE2 is directly related to two criteria: (1) is the Sumas fault an active fault, and (2) if it is, does the surface trace(s) of the fault (or projected surface trace if it is buried by geologic deposits that post-date the most recent surface rupture) cross the foot print of the proposed plant. With respect to item

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1, I have previously indicated the criteria for an active fault that are typically used, and I presented the lack of evidence that the Sumas fault meets the criteria. In addition, in his 1996 report, Dr. Clague — one of the authors of the map reprinted in Dr. Easterbrook's testimony — discusses the lack of a potential surface fault rupture hazard in southwestern British Columbia, including the portion of the Sumas Valley that is in Canada, and states that "there are no known active faults in the region and no known instances of ground rupture during historical earthquakes." My recent telephone conversation with him confirmed that to his knowledge, there is no existing geologic evidence that either the Sumas fault or Vedder Mountain fault are active faults that displace Holocene or latest Pleistocene deposits.

Despite Dr. Easterbrook's suggestion to the contrary, I am fully aware that surface fault ruptures can be a very narrow single trace or consist of multiple, subparallel breaks. In most cases, the multiple breaks occur within a few tens of feet from the primary rupture. However, I also know that the standard of practice is to identify potential fault traces based on surface (tectonic geomorphology) and/or subsurface exploration (e.g. trenching, geophysics, or drilled soil borings), and to set back structures planned for human occupancy or other critical functions a minimum of 50 feet from the identified trace(s) of an active fault.

As previously stated, there is currently no surface or shallow subsurface evidence that the projected surface trace of the Sumas fault transects the SE2 site. If the fault did experience displacement during the late Pleistocene or Holocene, its subsurface location most likely occurs at the interface between the Tertiary sedimentary rocks

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that bound the basin and the Quaternary sediments that fill the basin as shown in Exhibits MM-6 and MM-7 of my prefile testimony. If this fault were to rupture to the surface the most likely projection of the primary rupture would be considerably more than 50 feet west of the plant site and secondary ruptures, if any, would be also. In fact, according to the maps and diagrams provided by Dr. Easterbrook, the surface trace would be 2,000 feet or more from the SE2 site. This is shown in Exhibit MM-4 to my earlier prefiled direct testimony.

<u>7. Potential for landslides</u>.

I was fully aware that Dr. Easterbrook and his colleagues had published abstracts with information on large landslides in the Nooksack River drainage, and I have no disagreement with their conclusions regarding the potential for these landslides to have been seismically induced. However, I don't believe these have direct bearing on the potential landslide hazard at the SE2 site for reasons indicated in my prior testimony (reiterated below), and I would characterize the potential for a large landslide from Vedder Mountain to reach the SE2 site as very low to remote.

The geologic conditions of the slopes within the Nooksack River drainage area differ from Vedder Mountain, the Sumas upland, and the Sumas Valley. In addition, large landslides in the Nooksack that reach the river can be further mobilized by the river and funneled downstream within the river valley such as the lahar mapped in the Middle Fork by Easterbrook and Kovanen (1986). The published geologic mapping of Vedder Mountain and the Sumas Valley (including Dr. Easterbrook's) are of sufficient detail to recognize any large landslides that may have occurred subsequent

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to the Sumas glacial advance. The landslides that are shown on these maps do not extend much beyond the base of the mountain front. In the event that a large landslide from Vedder Mountain were to occur, it would have to cross the Sumas river and two creeks as well as the 2½ miles of flat valley floor to reach the SE2 site. I believe this is a very unlikely event and even if it did occur, the direct impact of the landslide on the intervening area would far exceed any secondary effects from impact to the plant.

8. Mitigation through engineering design.

I am not an engineer and will defer to Allan Porush's testimony for the specifics of design measures to mitigate potential liquefaction and ground shaking hazards. However, my experience working with highly qualified structural and earthquake engineers on numerous seismic hazard projects and my review of numerous published accounts of earthquake induced damage have provided me with the knowledge that structures properly designed and constructed in accordance with the modern U.S. building codes, including consideration of the properties of the underlying soils, perform well in earthquakes and rarely "fail."

9. Limited "new" information.

I did not mean to trivialize Dr. Easterbrook's work, and I agree that new research typically builds upon prior work. However, the new information he claims to have obtained as part of his research is not presented in his preliminary report, nor was it provided at our June 2000 meeting. Therefore I cannot assess it with respect to Dr. Easterbrook's opinions or include it in my evaluation of potential seismic hazards at

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the SE2 site. For example, he does not present any new subsurface information to assess the subsurface extent of the faults. The typical information of this type would be high resolution geophysical survey data, geologic logs of new subsurface borings, or re-interpretation of existing subsurface data such as that presented by Jones (1996) and Cox and Kahle (1999). The extent of the faults shown on the maps provided in the preliminary report is essentially the same as the maps prepared by Mustard and Rouse (1994), Mustard *et al.* (1998), and Clague (1998). He does not provide specific age data for sediments in the lower portion of the basin, nor specific estimates of the fault displacement other than his conclusion that the total depth of the basin is due to geologically recent faulting. Similarly, as mentioned above, Dr Engebretson's reference to the University of Washington Geophysics Department's web site does not provide any new or specific information to substantiate their conclusions.

<u>12. Surface displacement</u>.

As indicated in my prefiled direct testimony, during our meeting Dr. Easterbrook did indicate two locations on Figure 1 of his preliminary report where he identified east-west escarpments that he suspects are fault related. These east-west trending escarpments are located west of the northeast-southwest trending trace of the Sumas fault shown on Figure 1 as well as the published maps of Mustard and Rouse (1994), Mustard and others (1998) and Clague (1998). He also indicated he had not investigated these yet.

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The statement I made on Page 13 of my prefiled direct testimony specifically refers to the questions I posed to Dr. Easterbrook in our June meeting, namely: (1) is there evidence of displacement of Sumas deposits mapped by him southwest of the SE2 site that are crossed by his inferred trace of the Sumas fault shown on his Figure 1; (2) is there evidence of displacement of Sumas deposits mapped by Dragovich *et al*. (1997) that are crossed by his inferred trace of the Vedder Mountain fault shown on his Figure 1; and (3) was he aware of any evidence of displacement of Sumas or younger deposits along either fault in Canada. He answered that to his knowledge, there was not evidence of surface displacement at any of these locations.

13 & 14. Evidence from bedrock and correlation of rock age and type.

The point of my testimony, which Dr. Easterbrook does not acknowledge, is that the difference in elevation of bedrock can be caused by erosion as well as tectonic deformation (folding, faulting, and uplift and subsidence). This too is a fundamental concept of geology. Without specific evidence that there has been faulting during the Quaternary, which Dr. Easterbrook has failed to demonstrate, it cannot be concluded that the elevation difference on the bedrock surface is solely due to Quaternary age faulting and not erosion, or a combination of faulting and erosion.

In order to determine the amount of displacement on a fault, a contact between specific geologic units, rocks of the same lithology and age, or another geologic feature (*e.g.*, erosional surface, paleochannel, older fault, dike, etc.) that was originally contiguous on both sides of the fault must be identified. Based on the geometry of the fault and the displaced feature, the amount of displacement can be

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measured or estimated. Dr Easterbrook provides no specific evidence that demonstrates that the rocks directly beneath the unconsolidated sediments were previously at the same relative elevation as the rocks exposed on top of Vedder Mountain. Consequently, he cannot demonstrate that the relief in the bedrock surface is due to faulting or that the faulting occurred during the Quaternary. In fact, subsurface data for the Sumas Valley indicates that there has been “dip-slip offsets of only a few hundred metres and appear to be confined to the middle Tertiary and older strata (pre-Pliocene?).” Mustard and Rouse, 1994. Thus, the data suggest that the total amount of displacement on the Vedder Mountain and Sumas faults is similar to the 1,000 feet (300 meters) depth of Sumas Valley, and this displacement occurred more than 1.6 million years ago (pre-Quaternary) and probably during the Miocene (Mustard and Rouse, 1994).

Throughout most of the Fraser Valley and Puget Sound, Quaternary age deposits overlie Miocene or older rocks. The general lack of latest Miocene and Pliocene age rocks indicates that there was a period of at least several million years where much of the region, including the Sumas Valley, was undergoing erosion. Therefore it is a very logical interpretation that much if not all of the subsurface relief on the bedrock surface beneath Sumas Valley is due to pre-Quaternary erosion and displacement on the faults, and Quaternary erosion processes as discussed below in Response 16.

<u>15 & 18. Whether the Sumas Valley is a graben</u>.

As previously stated, based on the additional Canadian references I have reviewed since my direct prefiled testimony, I now concur that several hundred meters of

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displacement on the Sumas and Vedder Mountain faults formed a graben in the Tertiary sedimentary rocks underlying Sumas Valley. However as I also have previously indicated, the available data indicates that this displacement occurred more than 1.6 million years ago (pre-Quaternary) and probably during the Miocene (Mustard and Rouse, 1994). As indicated in Response 16 below, there is a several million year hiatus in the depositional history of the region when erosion of the underlying rock was occurring. In addition, there is currently no available data that indicate that there is an active graben within the Quaternary sediments that underlie Sumas Valley.

16. Glacial erosion.

As previously described in my responses to points 13 and 14, pre-Quaternary erosion combined with the several hundred meters of displacement of the Miocene rocks on the Vedder Mountain and Sumas faults indicated by Mustard and Rouse (1994) can account for most if not all the paleotopography on the bedrock surface beneath Sumas Valley. In addition, as Dr. Easterbrook and other researchers have indicated, there have been at least three and as many as six glacial advances in the Puget Sound region during the Quaternary (approximately last 1.6 million years). It should also be noted that during several of the glacial advances, the worldwide sea level was more than 400 feet lower than current sea level (Lajoie, 1986), and marine deposits are present in the subsurface of Sumas Valley in Canada (Cameron, 1989). This indicates the Sumas Valley was both well above and below sea level during the Quaternary. During this time period, glacial, sub-glacial and non-glacial erosion processes have also occurred that could have contributed to the formation of the subsurface

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paleotopography on the bedrock as well as the surface topography. Cameron (1989) characterized the surface morphology of Sumas Valley in Canada as a glacially scoured trough filled with sediments, with the bedrock bounding the valley grooved and striated by southwest flowing glacier.

I did not suggest that 1,000 feet of erosion was solely the result of glacial and subglacial erosion, or that it was only from one glacial advance and recession. As astonished as Dr. Easterboork is regarding my statements, I am equally astonished that he discounts the erosional capacity of glaciers. One need only go to Alaska to see numerous glaciers in valleys with similar width and similar or greater topographic relief to Sumas Valley. These are modern analogies to the Sumas Valley when it contained glacial ice during multiple Pleistocene glacial advances, and these modern examples are actively eroding metamorphic and igneous rocks that are probably similar to or harder than rocks beneath and on the slopes bounding Sumas Valley. Some of these valleys are along pre-existing faults while others have no evidence of a fault beneath or bounding the valley. However, to my knowledge few if any are grabens bounded by active faults.

An example is Port Valdez, a 3-mile wide, glacially-carved fjord where I and several co-workers spent three months mapping the bedrock geology, assessing the potential seismic hazards, and evaluating the late Pleistocene and Holocene glacial history. Port Valdez was formerly ice-filled but currently, glaciers are only present in tributary canyons to the main basin. It is partially filled with sediments and there is no evidence of faults beneath or bounding the basin. The current bathymetry of the fjord is a flat-bottomed trough approximately 800 feet deep. At the narrow mouth of the fjord, the bathymetric depth increases to 1,200 feet. The metamorphic rock bounding the basin is a very hard, and the overall topographic relief and steepness of most of the slopes bounding the basin are greater than those on Vedder and Sumas Mountains that bound Sumas Valley.

My opinion is still that erosion in the hiatus between deposition of the Miocene age rocks beneath Sumas Valley and the deposition of the Quaternary valley fill, in conjunction with the Quaternary erosional processes previously identified, could account for the paleotopography without Quaternary faulting at the basin margins. This opinion is based upon the information presented in my direct prefiled testimony, discussions with other knowledgeable geologists, and observations of analogous modern glaciers. The validity of this opinion was substantiated by my general conversations with Dr. Hallett regarding glacial erosion in the Puget Sound, as well as my specific conversation regarding the glacial and tectonic history of Sumas Valley with Dr. Clague (both of whom are described above, pp. 2:47-3:5).

<u>17. Evidence of a graben to the southwest</u>.

Dr. Easterbrook's response does not accurately reflect the data on which my testimony was based. In Exhibits MM-6 and MM-7 to my prefiled direct testimony, geologic cross sections by Cox and Kahle (1999) are reproduced which were used to assess the subsurface nature and extent of the graben. The number and depth of wells used by Cox and Kahle (1999) to construct their cross sections are greater to the southwest (Sections E-E' and F-F') relative to the northeast near the SE2 site

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(Sections A-A' and B-B'). In addition, geophysical and well data was used by Jones (1996) to contour the bedrock surface throughout the area. Jones's map was also used by Cox and Kahle. The oil wells used by Mustard and Rouse (1994) to construct the cross-section shown in Exhibit MM-2 are also located west and southwest of Sumas.

19. Distinctiveness of the Sumas Valley.

Drs. Easterbrook and Engebretson's opinions regarding the uniqueness of the degree of seismic hazards in Sumas valley are predicated on their opinions regarding the nature and degree of the individual hazards. As shown in my and Allan Porush's direct and rebuttal testimony, the hazards are either remote or not as severe as portrayed by Drs. Easterbrook and Engebretson. On pages 12 to 14 of this testimony, I further explain why the conditions of the Sumas Valley are not unique to the region.

20. Liquefaction.

My disagreement with Dr. Easterbrook is with his characterization that liquefaction would definitely occur at the SE2 site and be so severe and deep that it precludes use of any ground improvement techniques or foundation design. This has been addressed in my and Allan Porush's direct testimony, my introductory responses in this testimony, and in Mr. Porush's rebuttal testimony.

21 & 22. Evidence of surface displacement and criteria for determining fault activity.

As previously noted above, there are numerous studies and compilations that show that significant, primary surface fault ruptures associated with large historical

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earthquakes in the western U.S. (as well as other areas of the world with similar tectonic settings) have all occurred on faults with evidence of prior surface rupture in the Holocene or latest Pleistocene. Studies have been conducted on many of these faults to assess the timing and nature of pre-historical earthquakes (paleoseismology), and these show that recurrence intervals for surface rupturing events on these faults typically range from hundreds to several thousand years. Therefore, lack of evidence for displacement or deformation of Holocene age deposits is a well established basis for assessing potential fault rupture hazard and has been long established as a regulation in California. Granted, at some time in geologic history a fault has to form. However, this is most likely to occur when major changes in the tectonic stress regime occur, which is not the case during the Quaternary in the Pacific Northwest. There are hundreds of mapped faults in the region and only a few have been shown to be active during the current tectonic regime. Thus, based on the currently available data, there is not a significant fault rupture hazard at the SE2 site.

Dr. Easterbrook's comment regarding the two locations with potential surface displacement is addressed in Response 12 above.

Q. Thank you for all those specifics. Would you like to sum up your rebuttal to Professors Easterbrook and Engebretson?

A. Yes. I have provided specific information and reasons that support my conclusions regarding the potential seismic hazards at the SE2 site, namely that: (1) the potential for seismically induced landsliding or surface fault rupture to affect the SE2 site is remote; and (2) the expected strong ground motion and potential for liquefaction are

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not significantly greater than other areas of the region with similar geologic conditions and should be able to be adequately quantified and mitigated through proper design and construction measures. In contrast, Drs. Easterbrook and Engebretson have: (1) not provided sufficient evidence that seismically produced landslides or surface fault rupture are a likely hazard at the site; (2) exaggerated the potential risk of strong ground motio and liquefaction; and (3) dismissed the current standards of practice for evaluating and designing for these potential hazards. They have thus failed to substantiate their opinion that due to seismic concerns, the SE2 site is inappropriate as a location for a power facility such as has been proposed by SE2.

END OF TESTIMONY

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